ADDRESS

OF

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PRESIDENT.

My Lords, Ladies, and Gentlemen,

As this is the first time that the British Association for the Advancement of Science has met in the City of Exeter, and it is probable that many now present have never attended a former Meeting, I hope the older members of the Association will bear with me if I say a few words in explanation of the objects for which the Association was instituted. In the first place, then, it aims at fulfilling an office which is quite distinct from that of the various scientific societies which are established in different parts of the country. These, for the most part, have for their leading object to make the voluntary labours of isolated workers in science available to the scientific world generally by receiving, discussing, and publishing the results which they may have obtained. The British Association, on the other hand, aims at giving *a more systematic direction* to scientific inquiry, and that in various ways.

In a rapidly progressing branch of science it is by no means easy to become acquainted with its actual state. The workers in it are scattered throughout the civilized world, and their results are published in a variety of Transactions and scientific periodicals, mixed with other scientific matter. To make oneself, without assistance, well acquainted with what has been done, it is requisite to have access to an extensive library, to be able to read with facility several modern languages, and to have leisure to hunt through the tables of contents, or at least the indices, of a number of serial works. Without such knowledge, there is always the risk that a scientific man may spend his strength in doing over again what has been done already; whereas with better direction the same expenditure of time and labour might have resulted in some substantial addition to our knowledge. With a view to meet this difficulty, the British Association has requested individuals who were more specially conversant with particular departments of science, to draw up reports on the present state of our knowledge in, or on the recent progress of, special branches; and the influence of the Association as a public body has been found sufficient to induce a number of scientific men to undertake the great labour of preparing such reports.

By thus ascertaining thoroughly what we already had, what we still wanted was made more clear; and, indeed, it was one special object of the reports I have mentioned to point out what were the more prominent desiderata in the various subjects to which they related. The Association was thus the better enabled to fulfil another of its functions, that of organizing means for the prosecution of researches which require cooperation. When the want is within the compass of what can be accomplished by individuals, the demand may be left to create the supply; but it often happens that a research can hardly be carried out without cooperation. It may, for instance, require a combination of the most profound theoretical knowledge with the greatest experimental skill, or an extensive knowledge of very dissimilar branches of science; or, again, the work to be done, though all of one kind, may be of such an extent as to be beyond the power of any one man. In such cases the limited power of the individual can only be supplemented by the principle of cooperation; and accordingly it becomes an important part of the business of the Association to organize committees for the prosecution of special researches. The researches thus undertaken at the request of the Association are published at length, along with the reports on the progress of science, in the first part of the annual volume.

In close connexion with the last must be mentioned another mode in which the Association contributes to the progress of science. Many researches require not only time and thought, but pecuniary outlay; and it would seem hard that scientific men who give their time and labour gratuitously to carrying out such researches should be further obliged to incur an expenditure which they often can ill afford. The Association accordingly makes grants of money to individuals or Committees for defraying the expenses of such researches. It appears from the report which has just been published that, reckoning up to the year 1867 inclusive, the sum of £29,312 4*s.* 1*d.* has been voted by the Association for various scientific objects. Deducting from this the sum of £23 16*s.* 0*d.* for the balances of grants not wholly expended, which were returned to the Association, we may say that £29,288 8*s.* 1*d.* has been expended in the manner indicated. When we remember that these grants were mostly of small amount, and do not include personal expenses, and that very many of the researches undertaken at the request of the Association do not involve money grants at all, we may form some idea of the amount of scientific activity which has been evoked under the auspices of the Association.

In the address with which the business of the Meeting is opened, it is usual for your President to give some account of the most recent progress of science. The task is by no means an easy one. Few indeed are familiar with science in all its branches; and even to one who was, the selection of topics and the mode of treating them would still present difficulties. I shall not attempt to give an account of the recent progress of science in general, but shall select from those branches with which I am more familiar some examples of recent progress which may, I hope, prove to be of pretty general interest. And even in this I feel that I shall have to crave your indulgence, for it is hard to be intelligible to some without being wearisome to others.

Among the various branches of physical science, astronomy occupies in many respects a foremost rank. The movements of the heavenly bodies must have occupied the attention and excited the interest of mankind from the earliest ages, and accordingly the first rudiments of the science are lost in the depths of antiquity. The grandeur of the subjects of contemplation which it presents to us have won for it especial favour, and its importance in relation to navigation has caused it to be supported by national resources. Newton's great discovery of universal gravitation raised it from the rank of a science of observation to that of one admitting of the most exact mathematical deduction; and the investigation of the consequences of this law, and the explanation thereby of the lunar and planetary disturbances, have afforded a field for the exercise of the highest mathematical powers on the part of Newton and his successors. Gradually the apparent anomalies, as they might have been deemed, in the motions of the heavenly bodies were shown to be necessary consequences of the one fundamental law; and at last, as the result of calculations of enormous labour, tables were constructed enabling the places of those bodies at any given time to be determined years beforehand with astonishing precision. A still more striking step was taken. When it had been shown by careful calculation that the apparent motion of the remotest of the planets then known to belong to our system could not be wholly explained on the theory of gravitation, by taking account of the disturbing powers of the other known planets, Adams in our own country, and Le Verrier in France, boldly reversed the problem, and instead of determining the disturbing effect of a known planet, set themselves to inquire what must be the mass and orbit of an unknown planet which shall be capable of producing by its disturbing force the unexplained deviations in the position of Uranus from its calculated place. The result of this inquiry is too well known to require notice.

After these brilliant achievements, some may perhaps have been tempted to imagine that the field of astronomical research must have been well-nigh exhausted. Small perturbations, hitherto overlooked, might be determined, and astronomical tables thereby rendered still more exact. New asteroids might be discovered by the telescope. More accurate values of the constants with which we have to deal might be obtained. But no essential novelty of principle was to be looked for in the department of astronomy; for such we must go to younger and less mature branches of science.

Researches which have been carried on within the last few years, even the progress which has been made within the last twelve months, shows how short-sighted such an anticipation would have been; what an unexpected flood of light may sometimes be thrown over one science by its union with another; how conducive accordingly to the advancement of science may be an Association like the present, in which not only are the workers at special sciences brought together in the Sectional Meetings, but in the General Meetings of the Association, and in the social intercourse, which, though of an informal character, is no unimportant part of our procedings, the cultivators of different branches of science are brought together, and have an opportunity of enlarging their minds by contact with the minds of others, who have been used to trains of thought of a very different character from their own.

The science of astronomy is indebted to that of optics for the principles whch regulate the construction of those optical instruments which are so essential to the astronomer. It repaid its debt by furnishing to optics a result which it is important we should keep in view in considering the nature of light. It is to astronomy that we are indebted for the first proof we obtained of the finite velocity of light, and for the first numerical determination of that enormous velocity. Astronomy, again, led, forty-four years later, to a second determination of that velocity in the remarkable phenomenon of aberration discovered by Bradley, a phenomenon presenting special points of interest in relation to the nature of light, and which has given rise to some discussion, extending even to the present day, so that the Astronomer Royal has not deemed it unworthy of investigation, laborious as he foresees the trial is likely to prove, to determine the constant of aberration by means of a telescope having its tube filled with water.

If in respect of these phenomena optics received much aid from astronomy, the latter science has been indebted to the former for information which could not otherwise have been obtained. The motions and the masses of the heavenly bodies are revealed to us more or less fully by astronomical observations; but we could not thus become acquainted with the chemical nature of these distant objects. Yet, by the application of the spectroscope to the scrutiny of the heavenly bodies, evidence has been obtained of the existence therein of various elements known to us by the chemical examination of the materials of which our own earth is composed; and not only so, but light is thrown on the state in which matter is there existing, which, in the case of nebulæ especially, led to the formation of new ideas respecting their constitution, and the rectification of astronomical speculations previously entertained. I shall not, however, dwell further on this part of the subject, which is now of some years' standing, and has been mentioned by more than one of your former Presidents, but will pass on to newer researches in the same direction.

We are accustomed to apply to the stars the epithet *fixed.* Night after night they are seen to have the same relative arrangement; and when their places are determined by careful measurement, and certain small corrections due to known causes are applied to the immediate results of observation, they are found to have the same relative distances. But when instead of days the observations extend over months or years, it is found that the fixity is not quite absolute. Defining as fixity invariability of position as estimated with reference to the stars as a whole, and comparing the position of any individual star with those of the stars in its neighbourhood, we find that some of the stars exhibit "proper motions," show, that is, a progressive change of angular position as seen from the earth, or rather as they would be seen from the sun, which we may take for the mean annual place of the earth. This indicates linear motion in a direction transverse to the line joining the sun with the star. But since our sun is merely a star, a line drawn from the star exhibiting proper motion to our sun is, as regards the former, merely a line drawn to a star taken at random, and therefore there is no reason why the star's motion should be, except accidentally, in a direction perpendicular to the line joining the star with our sun. We must conclude that the stars, including our own sun, or some of them at least, are moving in various directions in space, and that it is merely the transversal component of the whole motion, or rather of the motion relatively to our sun, that is revealed to us by a change in the star's apparent place.

How then shall we determine whether any particular star is approaching to or receding from our sun? It is clear that astronomy alone is powerless to aid us here, since such a motion would be unaccompanied by change of angular position. Here the science of optics comes to our aid in a remarkable manner.

The pitch of a musical note depends, as we know, on the number of vibrations which reach the ear in a given time, such as a second. Suppose, now, that a body, such as a bell, which is vibrating a given number of times per second, is at the same time moving from the observer, the air being calm. Since the successive pulses of sound travel all with the velocity of sound, but diverge from different centres, namely, the successive points in the bell's path at which the bell was when those pulses were first excited, it is evident that the sound-waves will be somewhat more spread out on the side from which the bell is moving, and more crowded together on the side towards which it is moving, than if the bell had been at rest. Consequently the number of vibrations per second which reach the ear of an observer situated in the former of these directions will be somewhat smaller, and the number which reach an observer situated in the opposite direction somewhat greater, than if the bell had been at rest. Hence to the former the pitch will be somewhat lower, and to the latter somewhat higher, than the natural pitch of the bell. And the same thing will happen if the observer be in motion instead of the bell, or if both be in motion; in fact, the effect depends only on the *relative* motion of the observer and the bell in the direction of a line joining the two,—in other words, on the velocity of recession or approach of the observer and the bell. The effect may be perceived in standing by a railway when a train in which the steam-whistle is sounding passes by at full speed, or better still, if the observer be seated in a train which is simultaneously moving in the opposite direction.

The present state of optical science is such as to furnish us with evidence, of a force which is perfectly overwhelming, that light consists of a tremor or vibratory movement propagated in an elastic medium filling the planetary and stellar spaces, a medium which thus fulfils for light an office similar to that of air for sound. In this theory, to difference of periodic time corresponds difference of refrangibility. Suppose that we were in possession of a source of light capable, like the bell in the analogous case of sound, of exciting in the æther supposed at rest vibrations of a definite period, corresponding, therefore, to light of a definite refrangibility. Then, just as in the case of sound, if the source of light and the observer were receding from or approaching to each other with a velocity which was not insensibly small compared with the velocity of light, an appreciable lowering or elevation of refrangibility would be produced, which would be capable of detection by means of a spectroscope of high dispersive power.

The velocity of light is so enormous, about 185,000 miles per second, that it can readily be imagined that any motion which we can experimentally produce in a source of light is as rest in comparison. But the earth in its orbit round the sun moves at the rate of about 18 miles per second; and in the motions of stars approaching to or receding from our sun we might expect to meet with velocities comparable with this. The orbital velocity of the earth is, it is true, only about the one ten-thousandth part of the velocity of light. Still the effect of such a velocity on the refrangibility of light, which admits of being easily calculated, proves not to be so insensibly small as to elude all chance of detection, provided only the observations are conducted with extreme delicacy.

But how shall we find in such distant objects as the stars an analogue of the bell which we have assumed in the illustration drawn from sound? What evidence can we ever obtain, even if an examination of their light should present us with rays of definite refrangibility, of the existence in those remote bodies of ponderable matter vibrating in known periods not identical with those corresponding to the refrangibilities of the definite rays which we observe? The answer to this question will involve a reference, which I will endeavour to make as brief as I can, to the splendid researches of Professor Kirchhoff. The exact coincidence of certain dark lines in the solar spectrum with bright lines in certain artificial sources of light had previously been in one or two instances observed; but it is to Kirchhoff we owe the inference from an extension of Prevost's theory of exchanges, that a glowing medium which emits bright light of any particular refrangibility *necessarily* (at that temperature at least) acts as an absorbing medium, extinguishing light of the same refrangibility. In saying this it is but just to mention that in relation to radiant heat (from whence the transition to light is easy), Kirchhoff was preceded, though unconsciously, by our own countryman Mr. Balfour Stewart. The inference which Kirchhoff drew from Prevost's theory thus extended led him to make a careful comparison of the places of the dark lines of the solar spectrum with those of bright lines produced by the incandescent gas or vapour of known elements; and the coincidences were in many cases so remarkable as to establish almost to a certainty the existence of several of the known elements in the solar atmosphere, producing by their absorbing action the dark lines coinciding with the bright lines observed. Among other elements may be mentioned in particular hydrogen, the spectrum of which, when traversed by an electric discharge, shows a bright line or band exactly coinciding with the dark line C, and another with the line F.

Now Mr. Huggins found that several of the stars show in their spectra dark] lines coinciding in position with C and F; and what strengthens the belief that this coincidence, or apparent coincidence, is not merely fortuitous, but is due to a common cause, is that the two lines are found associated together, both present or both absent. And Kirchhoff's theory suggests that the common cause is the existence of hydrogen in the atmospheres of the sun and certain stars, and its exercise of an absorbing action on the light emitted from beneath.

Now by careful and repeated observations with a telescope furnished with a spectroscope of high dispersive power Mr. Huggins found that the F line, the one selected for observation, in the spectrum of Sirius did not exactly coincide with the corresponding bright line of a hydrogen spark, which latter agrees in position with the solar F, but was a *little* less refrangible, while preserving the same general appearance. What conclusion, then, are we to draw from the result? Surely it would be most unreasonable to attribute the dark lines in the spectra of the sun and of Sirius to distinct causes, and to regard their almost exact coincidence as purely fortuitous, when we have in proper motion a *vera causa* to account for a minute difference. And if, as Kirchhoff's labours render almost certain, the dark solar line depends on the existence of hydrogen in the atmosphere of our sun, we are led to infer that that element, with which the chemist working in his laboratory is so familiar, exists and is subject to the same physical laws in that distant star, so distant, that, judging by the most probable value of its annual parallax, light which would go seven times round our earth in one second would take fourteen years to travel from the star. What a grand conception of the unity of plan pervading the universe do such conclusions present to our minds!

Assuming, then, that the small difference of refrangibility observed between the solar F and that of Sirius is due to proper motion, Mr. Huggins concludes from his measures of the minute difference of position that at the time of the observation Sirius was receding from the earth at the rate of 41·4 miles per second. A part of this was due to the motion of the earth in its orbit; and on deducting the orbital velocity of the earth, resolved in the direction of a line drawn from the star, there remained 29·4 miles per second as the velocity with which Sirius and our sun are mutually receding from each other. Considering the minuteness of the quantity on which the result depends, it is satisfactory to find that Mr. Huggins's results as to the motion of Sirius have been confirmed by the observations of Father Secchi made at Rome with a different instrument.

The determination of radial proper motion in this way is still in its infancy. It is worthy of note that, unlike the detection of transversal proper motion by change of angular position, it is equally applicable to stars at all distances, provided they are bright enough to render the observations possible. It is conceivable that the results of these observations may one day lead to a determination of the motion of the solar system in space, which is more trustworthy than that which has been deduced from changes of position, as being founded on a broader induction, and not confined to conclusions derived from the stars in our neighbourhood. Should even the solar system and the nearer stars be drifting along, as Sir John Herschel suggests, with an approximately common motion, like motes in a sunbeam, it is conceivable that the circumstance might thus be capable of detection. To what wide speculations are we led as to the possible progress of our knowledge when we put together what has been accomplished in different branches of science!

I turn now to another recent application of spectral analysis. The phenomenon of a total solar eclipse is described by those who have seen it as one of the most imposing that can be witnessed. The rarity of its occurrence and the shortness of its duration afford, however, opportunity for only a hasty study of the phenomena which may then present themselves. Among these, one of the most remarkable, seen indeed before, but first brought prominently into notice by the observers who watched the eclipse of July 7, 1842, consists in a series of mountain-like or cloud-like luminous objects seen outside the dark disk of the moon. These have been seen in subsequent total eclipses, and more specially studied, by means of photography, by Mr. Warren De La Rue in the eclipse of June 18, 1860. The result of the various observations, and especially the study, which could be made at leisure, of the photographs obtained by Mr. De La Rue, proved conclusively that these appendages belong to the sun, not to the moon. The photographs proved further their light to be remarkable for actinic power. Since that time the method of spectral analysis has been elaborated; and it seemed likely that additional information bearing on the nature of these objects might be obtained by the application of the spectroscope. Accordingly various expeditions were equipped for the purpose of observing the total solar eclipse which was to happen on

August 17, 1868. In our own country an equatorially mounted telescope provided with a spectroscope was procured for the purpose by the Royal Society, which was entrusted to Lieut. (now Captain) Herschel who was going out to India, one of the countries crossed by the line of the central shadow. Another expedition was organized by the Royal Astronomical Society, under the auspices of Major Tennant, who was foremost in pressing on the attention of scientific men the importance of availing themselves of the opportunity.

Shortly before the conclusion of the Meeting of the Association at Norwich last year, the first results of the observations were made known to the Meeting through the agency of the electric telegraph. In a telegram sent by M. Janssen to the President of the Royal Society, it was announced that the spectrum of the prominences was very remarkable, showing bright lines, while that of the corona showed none. Brief as the message necessarily was, one point was settled. The prominences could not be clouds in the strict sense of the term, shining either by virtue of their own heat, or by light reflected from below. They must consist of incandescent matter in the *gaseous* form. It appeared from the more detailed accounts received by post from the various observers, and put together at leisure, that except in the immediate neighbourhood of the sun the light of the prominences consisted mainly of three bright lines, of which two coincided with C and F, and the intermediate one nearly, but, as subsequent researches showed, not exactly, with D. The bright lines coinciding with C and F indicate the presence of glowing hydrogen. Several of the other lines were identified with those which would be produced by the incandescent vapour of certain other elements.

This is precious information to have gathered during the brief interval of the total phase, and required on the part of the observers self-denial in withdrawing the eye from the imposing spectacle of the surrounding scenery, and coolness in proceeding steadily with some definite part of the inquiry, when so many questions crowded for solution, and the fruits of months of preparation were to be reaped in three or four minutes or lost altogether; especially when, as too often happened, the observations were provokingly interrupted by flying clouds.

But valuable as these observations were, it is obvious that we should have had long to wait before we could have became acquainted with the usual behaviour of these objects, and their possible relation to changes which may be going on at the surface of the sun, if we had been dependent on the rare and brief phenomenon of a total solar eclipse for gathering information respecting them. But how, the question might be asked, shall we ever be able so to subdue the overpowering glare of our great luminary, and the dazzling illumination which it produces in our atmosphere when we look nearly in its direction, as to perceive objects which are comparatively so faint? Here again the science of optics comes in aid of astronomy.

When a line of light, such as a narrow slit held in front of a luminous object, is viewed through a prism, the light is ordinarily spread out into a coloured band, the length of which may be increased at pleasure by substituting two or more prisms for the single prism. As the total quantity of light is not thereby increased, it is obvious that the intensity of the light of the coloured band will go on decreasing as the length increases. Such is the case with ordinary sources of light, like the flame of a candle or the sky, which give a continuous spectrum, or one generally continuous, though interrupted by dark bands. But if the light from the source be homogeneous, consisting, that is, of light of one degree of refrangibility only, the image of the slit will be merely deviated by the prisms, not widened out into a band, and not consequently reduced in intensity by the dispersion. And if the source of light emit light of both kinds, it will be easily understood that the images of the slit corresponding to light of any definite refrangibilities which the mixture may contain will stand out, by their superior intensity, on the weaker ground of the continuous spectrum.

Preparations for observations of the kind had long been in progress in the hands of our countryman Mr. Lockyer. His first attempts were unsuccessful: but undismayed by failure, he ordered the construction of a new spectroscope of superior power, in which he was aided by a grant from the sum placed annually by Parliament at the disposal of the Royal Society for scientific purposes. The execution of this instrument was delayed by what proved to be the last illness of the eminent optician to whom it was entrusted, the late Mr. Cooke; but when at last the instrument was placed in his hands, Mr. Lockyer was not long in discovering the object of his two years' search. On the 20th of October last year, in examining the space immediately surrounding the edge of the solar disk, he obtained evidence, by the occurrence of a bright line in the spectrum, that his slit was on the image of one of those prominences the nature of which had so long been an enigma. It further appeared from an observation made on November 5 (as indeed might be expected from the photographs of Mr. De La Rue, and the descriptions of those who had observed total solar eclipses) that the prominences were merely elevated portions of an extensive luminous stratum of the same general character, which, now that the necessity of the interposition of the moon was dispensed with, could be traced completely round the sun. Notices of this discovery were received from the author by the Royal Society on October 21 and November 3, and the former was almost immediately published in No. 105 of the Proceedings. These were shortly afterwards followed by a fuller paper on the same subject.

Meanwhile the same thing had been independently observed in another part of the world. After having observed the remarkable spectrum of the prominences during the total eclipse, it occurred to M. Janssen that the same method might allow the prominences to be detected at any time; and on trial he succeeded in detecting them the very day after the eclipse. The results of his observations were sent by post, and were received shortly after the account of Mr. Lockyer's discovery had been communicated by Mr. De La Rue to the French Academy.

In the way hitherto described a prominence is not seen as a whole, but the observer knows when its image is intercepted by the slit; and by varying a little the position of the slit a series of sections of the prominence are obtained, by putting which together the form of the prominence is deduced. Shortly after Mr. Lockyer's communication of his discovery, Mr. Huggins, who had been independently engaged in the attempt to render the prominences visible by the aid of the spectroscope, succeeded in seeing a prominence as a whole by somewhat widening the slit, and using a red glass to diminish the glare of the light admitted by the slit, the prominence being seen by means of the C line in the red. Mr. Lockyer had a design for seeing the prominences as a whole by giving the slit a rapid motion of small extent, but this proved to be superfluous, and they are now habitually seen with their actual forms. Nor is our power of observing them restricted to those which are so situated that they are seen by projection outside the sun's limb; such is the power of the spectroscopic method of observation that it has enabled Mr. Lockyer and others to observe them right on the disk of the sun, an important step for connecting them with other solar phenomena.

One of the most striking results of the habitual study of these prominences is the evidence they afford of the stupendous changes which are going on in the central body of our system. Prominences the heights of which are to be measured by thousands and tens of thousands of miles, appear and disappear in the course of some minutes. And a study of certain minute changes of position in the bright line F, which receive a simple and natural explanation by referring them to proper motion in the glowing gas by which that line is produced, and which we see no other way of accounting for, have led Mr. Lockyer to conclude that the gas in question is sometimes travelling with velocities comparable with that of the earth in its orbit. Moreover these exhibitions of intense action are frequently found to be intimately connected with the spots, and can hardly fail to throw light on the disputed question of their formation. Nor are chemical composition and proper motion the only physical conditions of the gas which are accessible to spectral analysis. By comparing the breadth of the bright bands (for though narrow they are not mere lines) seen in the prominences with those observed in the spectrum of hydrogen rendered incandescent under different physical conditions, Dr. Frankland and Mr. Lockyer have deduced conclusions respecting the pressure to which the gas is subject in the neighbourhood of the sun. I am happy to say that Mr. Lockyer has consented to deliver a discourse during our Meeting, in which the whole subject will doubtless be fully explained.

I have dwelt perhaps too long on this topic, and I cannot help fearing that I may have been tedious to the many scientific men to whom the subject is already perfectly familiar. Yet the contemplations which it opens out to us are so exalted, and the proof which it affords of what can be accomplished by the union of different branches of science is so striking, that I hope I may be pardoned for occupying your time. I cannot, however, leave the subject of Astronomy without congratulating the Association on the accomplishment of an object which originated with it, and in the promotion of which it formerly took an active part. It was at the Meeting of the Association at Birmingham in 1849, under the presidency of the Rev. Dr. Robinson, that a resolution was passed for making an application to Her Majesty's Government to establish a reflector of not less than three feet aperture at the Cape of Good Hope, and to make such additions to the staff of that observatory as might be necessary for its effectual working. This resolution met with the hearty concurrence of the President of the Council of the Royal Society, who suggested that the precise locality in the Southern hemisphere where the telescope should be erected had best be left an open question. This modification having been adopted by your Council, the application was presented to Earl Russell, then First Lord of the Treasury, by representatives of both bodies early in 1850. A reply was received from Government to the effect that though they agreed with the Association as to the interest which attached itself to the inquiry, yet there was so much difficulty attending the arrangements that they were not prepared to take any steps without much further enquiry. This reply was considered so far favourable as not to forbid the hope of success if the application were renewed on a suitable opportunity. The subject was again brought before the Association by Colonel (now General Sir Edward) Sabine, in his opening address as President at the Belfast Meeting in 1852. The result was that the matter was again brought before Government by a Committee of the British Association acting in conjunction with a Committee of the Royal Society, by means of an application made to the Earl of Aberdeen. By this time the country was engaged in the Russian war, in consequence of which, it was replied, no funds could then be spared; but a promise was given that when the crisis then impending was past, the matter should be taken up, a promise which the retirement from office and subsequent death of Lord Aberdeen rendered of no avail.

But though failing in its immediate object, the action of the British Association in this matter has not remained fruitless. A few years later the subject was warmly taken up at Melbourne, and after preliminary correspondence between the Board of Visitors of the Melbourne Observatory and the President and Council of the Royal Society, and the appointment by the latter body of a Committee to consider and report on the subject, in April 1864 a proposition was made to the Colonial Legislature for a grant of £5000 for the construction of a telescope, and was acceded to. Not to weary you with details, I will merely say that the telescope has been constructed by Mr. Grubb, of Dublin, and is now erected at Melbourne, and in the hands of Mr. Le Sueur, who has been appointed to use it. It is a reflector of four feet aperture, of the Cassegrain construction, equatorially mounted, and provided with a clock-movement. Before its shipment, it was inspected in Dublin by the Committee appointed by the Royal Society to consider the best mode of carrying out the object for which the vote was made by the Melbourne Legislature; and the Committee speak in the highest terms of its contrivance and execution. We may expect before long to get a first instalment of the results obtained by a scrutiny of the southern heavens with an instrument far more powerful than any that has hitherto been applied to them—results which will at the same time add to our existing knowledge and redound to the honour of the Colony, by whose liberality this long-cherished object has at last been effected.

As I have mentioned] an application to the Government on the part of the Association which was not successful, it is but right to say that such is not generally the result; I will refer to one instance. At the Cambridge Meeting of the Association in 1862, a Committee, consisting of representatives of the Mechanical and Chemical Sections, was appointed for the purpose of investigating the application of gun-cotton to warlike purposes. At the Newcastle Meeting, in the following year, this Committee presented their Report. It was felt that a complete study of the subject demanded appliances which could be obtained only from our military resources, and at the Newcastle Meeting a resolution was passed recommending the appointment of a Royal Commission. This recommendation was adopted, and in 1864 a Commision was appointed, which was requested to report on the application of gun-cotton to Civil as well as to Naval and Military purposes. The Committee gave in their report last year, and that report, together with a more recent return relative to the application of gun-cotton to mining and quarrying operations, has just been printed for the House of Commons.

A substance of such comparatively recent introduction cannot be fairly compared with an explosive in the use of which we have the experience of centuries. Yet, even with our present experience, there are some purposes for which gun-cotton can advantageously replace gunpowder, while its manufacture and storage can be effected with comparative safety, since it is in a wet state during the process of manufacture, and is not at all injured by being kept permanently in water, but merely requires to be dried for use. Even should it be required to store it in the dry state, it is doubtful whether, with the precautions indicated by the chemical investigations of Mr. Abel, any greater risk is incurred than in the case of gunpowder. In the blasting of hard rocks it is found to be highly efficient, while the remarkable results recently obtained by Mr. Abel leave no doubt of its value for explosions such as are frequently required in warfare. General Hay speaks highly of the promise of its value for small arms; but many more experiments are required, especially as a change in the arm and mode of ignition require a change in the construction of the cartridge. In heavy ordnance, the due control of the rapidity of combustion of the substance is a matter of greater difficulty; and, though considerable progress has been made, much remains to be done before the three conditions of safety to the gun, high velocity of projection, and uniformity of result, are satisfactorily combined.

By the kindness of Dr. Carpenter, I am enabled to mention to you the latest results obtained in an expedition which could not have been undertaken without the aid of Government, an aid which was freely given. Last year Dr. Carpenter and Professor Wyville Thomson represented to the President and Council of the Royal Society the great importance to Zoology and Palæontology of obtaining soundings from great depths in the ocean, and suggested to them to use their influence with the Admiralty to induce them to place a gun-boat, or other suitable vessel, at the disposal of those gentlemen and any other naturalists who might be willing to accompany them for the purpose of carrying on a systematic course of deep-sea dredging for a month or six weeks. This application was forwarded to the Admiralty with the warm support of the President and Council, and was readily acceded to. The operations were a good deal impeded by rough weather, but nevertheless important results were obtained. Dredging was successfully accomplished at a depth of 650 fathoms; and the existence was established of a varied and abundant submarine Fauna, at depths which had generally been supposed to be either azoic, or occupied by animals of a very low type; and the character of the Fauna and of the mud brought up was such as to point to a chalk formation actually going on.

It seemed desirable to carry the soundings to still greater depths, and to examine more fully the changes of temperature which had been met with in the descent. Another application was accordingly made to the Admiralty in the present year, and was no less readily acceded to than the former; and a larger vessel than that used last year is now on her cruize. I am informed by Dr. Carpenter that dredging has been successfully carried down to more than 2400 fathoms (nearly the height of Mont Blanc), and that animal life has been found even at that depth in considerable *variety*, though its *amount* and *kind* are obviously influenced by the reduction of temperature to Arctic coldness. A very careful series of temperature soundings has been taken, showing, on the same spot, a continuous descent of temperature with the depth, at first more rapid, afterwards pretty uniform. Thermometers protected from pressure by a plan described by Dr. Miller were found to maintain their character at the great depths reached, the difference between them and the best ordinary thermometers used in the same sounding being exactly conformable to the pressure corresponding with each depth, as determined by the experiments previously made in smaller depths. All the observations hitherto made go to confirm the idea of a general interchange of polar and equatorial water, the former occupying the lowest depths, the latter forming a superficial stratum of 700 or 800 fathoms. The analyses of the water brought up indicate a large proportion of carbonic acid in the gases of the deep waters, and a general diffusion of organic matter.

I must turn for a few moments to another application recently made to Government, which has not been successful. The application I have in view was made, not by the British Association or other Scientific Societies in their corporate capacity, but by a body composed of the Presidents of the British Association and of the Royal and other leading Scientific Societies; and its object was, not the promotion of Science directly, but the recognition of preeminent scientific merit. In the history of science few names, indeed, hold so prominent a place as that of Faraday. The perfect novelty of principle and recondite nature of many of his great discoveries are such as to bear the impress of genius of the highest order, and to form an epoch in the advance of science; and while his scientific labours excited the admiration of men of science throughout the world, his singularly genial disposition, and modest unassuming character, won for him the love of those who had the happiness of numbering him among their personal friends. At a meeting of the Presidents of the Scientific Societies to which I have alluded, it was resolved to erect a marble statue in memory of Faraday. He was a man of whom England may well be proud, and it was thought that it would be a graceful recognition of his merits if the monument were erected at the public expense. The present Chancellor of the Exchequer, however, did not think it right that the recognition of scientific merit, however eminent, should fall on the taxation of the country, though even in a pecuniary point of view the country has received so much benefit from the labours of scientific men. The carrying out of the resolution being thus left to private exertion, a public meeting, presided over by H.R.H. the Prince of Wales, was held in the Royal Institution, an establishment which has the honour of being identified with Faraday's scientific career. At this Meeting a Committee was formed to carry out the object, and a subscription list commenced. By permission of the Secretaries of this Association, an office has been opened in the reception-room, where those Members of the Association who may be desirous of taking part in the movement will have every facility afforded them.

In chemistry, I do not believe that any great step has been made within the last year; but perhaps there is no science in which an earnest worker is so sure of being rewarded by making some substantial acquisition to our knowledge, though it may not be of the nature of one of those grand discoveries which from time to time stamp their impress on different branches of science. I may be permitted to refer to one or two discoveries which are exceedingly curious, and some of which may prove of considerable practical importance.

The Turaco or Plantain-eater of the Cape of Good Hope is celebrated for its beautiful plumage. A portion of the wings is of a fine red colour. This red colouring-matter has been investigated by Professor Church, who finds it to contain nearly six per cent. of copper, which cannot be distinguished by the ordinary tests, nor removed from the colouring-matter without destroying it. The colouring-matter is in fact a natural organic compound of which copper is one of the essential constituents. Traces of this metal had previously been found in animals, for example, in oysters, to the cost of those who partook of them. But in these cases the presence of the copper was merely accidental; thus oysters that lived near the mouths of streams which came down from copper-mines assimilated a portion of the copper salt, without apparently its doing them either good or harm. But in the Turaco the existence of the red colouring-matter which belongs to their normal plumage is dependent upon copper, which, obtained in minute quantities with the food, is stored up in this strange manner in the system of the animal. Thus in the very same feather, partly red and partly black, copper was found in abundance in the red parts, but none or only the merest trace in the black.

This example warns us against taking too utilitarian a view of the plan of creation. Here we have a chemical substance elaborated which is perfectly unique in its nature, and contains a metal the salts of which are ordinarily regarded as poisonous to animals; and the sole purpose to which, so far as we know, it is subservient in the animal economy is one of pure decoration. Thus a pair of the birds which were kept in captivity lost their fine red colour in the course of a few days, in consequence of washing in the water which was left them to drink, the red colouring-matter, which is soluble in water, being thus washed out; but except as to the loss of their beauty it does not appear that the birds were the worse for it.

A large part of the calicos which are produced in this country in such enormous quantities are sent out into the market in the printed form. Although other substances are employed, the place which madder occupies among dye-stuffs with the calico-printer is compared by Mr. Schunck to that which iron occupies among metals with the engineer. It appears from the public returns that upwards of 10,000 tons of madder are imported annually into the United Kingdom. The colours which madder yields to mordanted cloth are due to two substances, alizarine and purpurine, derived from the root. Of these, alizarine is deemed the more important, as producing faster colours, and yielding finer violets. In studying the transformations of alizarine under the action of chemical reagents, MM. Graebe and Liebermann were led to connect it with anthracene, one of the coal-tar series of bodies, and to devise a mode of forming it artificially. The discovery is still too recent to allow us to judge of the cost with which it can be obtained by artificial formation, which must decide the question of its commercial employment. But assuming it to be thus obtained at a sufficiently cheap rate, what a remarkable example does the discovery afford of the way in which the philosopher quietly working in his laboratory may obtain results which revolutionize the industry of nations! To the calico-printer indeed it may make no very important difference whether he continues to use madder, or replaces it by the artificial substance; but what a sweeping change is made in the madder-growing interest! What hundreds of acres hitherto employed in madder-cultivation are set free for the production of human food, or of some other substance useful to man! Such changes can hardly be made without temporary inconvenience to those who are interested in the branches of industry affected; but we must not on that account attempt to stay the progress of discovery, which is conducive to the general weal.

Another example of the way in which practical applications unexpectedly turn up when science is pursued for its own sake is afforded by a result recently obtained by Dr. Matthiessen, in his investigation of the constitution of the opium bases. He found that by the action of hydrochloric acid on morphia a new base was produced, which as to composition differed from the former merely by the removal of one equivalent of water. But the physiological action of the new base was utterly different from that of the original one. While morphia is a powerful narcotic, the use of which is apt to be followed by subsequent depression, the new base was found to be free from narcotic properties, but to be a powerful emetic, the action of which was unattended by injurious after-effects. It seems likely to become a valuable remedial agent.

In relation to mechanism, this year is remarkable as being the centenary of the great invention of our countryman James Watt. It was in the year 1769 that he took out his patent involving the invention of separate condensation, which is justly regarded as forming the birth of the steam-engine. Little could even his inventive mind have foreseen the magnitude of the gift he was conferring on mankind in general, and on his own country more particularly. In these days of steamers, power-looms, and railways, it requires no small effort to place ourselves in imagination in the condition we should be in without the steam-engine. It needs no formal celebration to remind Britons of what they owe to Watt. Of him truly it may be said "*si monumentum requiras circumspice.*"

With reference to those branches of science in which we are more or less concerned with the phenomena of life, my own studies give me no right to address you. I regret this the less because my predecessor and my probable successor in the Presidential Chair are both of well-known eminence in this department. But I hope I may be permitted as a physicist, and viewing the question from the physical side, to express to you my views as to the relation which the physical bear to the biological sciences.

No other physical science has been brought to such perfection as mechanics; and in mechanics we have long been familiar with the idea of the perfect generality of its laws, of their applicability to bodies organic as well as inorganic, living as well as dead. Thus in a railway collision when a train is suddenly arrested the passengers are thrown forward, by virtue of the inertia of their bodies, precisely according to the laws which regulate the motion of dead matter. So trite has the idea become that the reference to it may seem childish ; but from mechanics let us pass on to chemistry, and the case will be found by no means so clear. When chemists ceased to be content with the mere ultimate analysis of organic substances, and set themselves to study their proximate constituents, a great number of definite chemical compounds were obtained which could not be formed artificially. I do not know what may have been the usual opinion at that time among chemists as to their mode of formation. Probably it may have been imagined that chemical affinities were indeed concerned in their formation, but controlled and modified by an assumed vital force. But as the science progressed many of these organic substances were formed artificially, in some cases from other and perfectly distinct organic substances, in other cases actually from their elements. This statement must indeed be accepted with one qualification. It was stated several years ago by M. Pasteur, and I believe the statement still remains true, that no substance the solution of which possesses the property of rotating the plane of polarization of polarized light had been formed artificially from substances not possessing that property. Now several of the natural substances which are deemed to have been produced artificially are active, in the sense of rotating the plane of polarization; and therefore in these cases the inactive, artificial substances cannot be absolutely identical with the natural ones. But the inactivity of the artificial substance is readily explained on the supposition that the artificial substance bears to the natural, the same relation as racemic acid bears to tartaric,—that it is, so to speak, a mixture of the natural substance with its image in a mirror. And when we remember by what a peculiar and troublesome process M. Pasteur succeeded in separating racemic acid into the right-handed and left-handed tartaric acids, it will be at once understood how easily the fact, if it be a fact, of the existence in the natural substance of a mixture of two substances, one right-handed and the other left-handed, but otherwise identical, may have escaped detection. This is a curious point, to the clearing up of which it is desirable that chemists should direct their attention. Waiving then the difference of activity or inactivity, which, as we have seen, admits of a simple physical explanation, though the correctness of that explanation remains to be investigated, we may say that at the present time a considerable number of what used to be regarded as essentially natural organic substances have been formed in the laboratory. That being the case, it seems most reasonable to suppose that in the plant or animal from which those organic substances were obtained they were formed by the play of ordinary chemical affinity, not necessarily nor probably by the same series of reactions by which they were formed in the laboratory, where a high temperature is commonly employed, but still by chemical reactions of some kind, under the agency in many cases of light, an agency sometimes employed by the chemist in his laboratory. And since the boundary line between the natural substances which have and those which have not been formed artificially is one which, so far as we know, simply depends upon the amount of our knowledge, and is continually changing as new processes are discovered, we are led to extend the same reasoning to the various chemical substances of which organic structures are made up.

But do the laws of chemical affinity, to which, as I have endeavoured to infer, living beings, whether vegetable or animal, are in absolute subjection, together with those of capillary attraction, of diffusion,and so forth, account for the formation of an organic structure, as distinguished from the elaboration of the chemical substances of which it is composed? No more, it seems to me, than the laws of motion account for the union of oxygen and hydrogen to form water, though the ponderable matter so uniting is subject to the laws of motion during the act of union just as well as before and after. In the various processes of crystallization, of precipitation, and so forth, which we witness in dead matter, I cannot see the faintest shadow of an approach to the formation of an organic structure, still less, to the wonderful series of changes which are concerned in the growth and perpetuation of even the lowliest plant. Admitting to the full as highly probable, though not completely demonstrated, the applicability to living beings of the laws which have been ascertained with reference to dead matter, I feel constrained at the same time to admit the existence of a mysterious *something* lying beyond,—a something *sui generis*, which I regard, not as balancing and suspending the ordinary physical laws, but as working with them and through them to the attainment of a designed end.

What this *something*, which we call life, may be, is a profound mystery. We know not how many links in the chain of secondary causation may yet remain behind; we know not how few. It would be presumptuous indeed to assume in any case that we had already reached the last link, and to charge with irreverence a fellow worker who attempted to push his investigations yet one step further back. On the other hand, if a thick darkness enshrouds all beyond, we have no right to assume it to be impossible that we should have reached even the last link of the chain; a stage where further progress is unattainable, and we can only refer the highest law at which we stopped to the fiat of an Almighty Power. To assume the contrary as a matter of necessity, is practically to remove the First Cause of all to an infinite distance from us. The boundary, however, between what is clearly known and what is veiled in impenetrable darkness is not ordinarily thus sharply defined. Between the two there lies a misty region, in which loom the ill-discerned forms of links of the chain which are yet beyond us. But the general principle is not affected thereby. Let us fearlessly trace the dependence of link on link as far as it may be given us to trace it, but let us take heed that in thus studying second causes we forget not the First Cause, nor shut our eyes to the wonderful proofs of design which, in the study of organized beings especially, meet us at every turn.

Truth we know must be self-consistent, nor can one truth contradict another, even though the two may have been arrived at by totally different processes, in the one case, suppose, obtained by sound scientific investigation, in the other case taken on trust from duly authenticated witnesses. Misinterpretations of course there may be on the one side or on the other, causing *apparent* contradictions. Every mathematician knows that in his private work he will occasionally by two different trains of reasoning arrive at discordant conclusions. He is at once aware that there must be a slip somewhere, and sets himself to detect and correct it. When conclusions rest on probable evidence, the reconciling of apparent contradictions is not so simple and certain. It requires the exercise of a calm, unbiassed judgment, capable of looking at both sides of the question; and oftentimes we have long to suspend our decision, and seek for further evidence. None need fear the effect of scientific enquiry carried on in an honest, truth-loving, humble spirit, which makes us no less ready frankly to avow our ignorance of what we cannot explain than to accept conclusions based on sound evidence. The slow but sure path of induction is open to us. Let us frame hypotheses if we will: most useful are they when kept in their proper place, as stimulating inquiry. Let us seek to confront them with observation and experiment, thereby confirming or upsetting them as the result may prove; but let us beware of placing them prematurely in the rank of ascertained truths, and building further conclusions on them as if they were.

When from the phenomena of life we pass on to those of mind, we enter a region still more profoundly mysterious. We can readily imagine that we *may* here be dealing with phenomena altogether transcending those of mere life, in some such way as those of life transcend, as I have endeavoured to infer, those of chemistry and molecular attractions, or as the laws of chemical affinity in their turn transcend those of mere mechanics. Science can be expected to do but little to aid us here, since the instrument of research is itself the object of investigation. It can but enlighten us as to the depth of our ignorance, and lead us to look to a higher aid for that which most nearly concerns our wellbeing.